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REPORT ON TRIP TO CALIFORNIA J. J. STOKER

Itinerary:

047

June 10-June 15: Traveling to Los Angeles by train.

June 16- Los Angeles. Visited North American Aircraft.

June 17-June 19: La Jolla. Working at Scripps Institution of Oceanography with Munk, Sverdrup, and Arthur. Also visited Robert of Naval Electronics Laboratory in San Diego.

June 20-June 21- Pasadena. Visited Knapp and Warner at Cal. Tech. and talked with Lieberman and Schmanblust.

June 22- Traveling from Los Angeles to Berkeley.

June 23- Spent most of the day with Johnson and O'Brien at Univ. of Cal. Engineering Div.

June 24-June 28- Took a vacation in the Sierras.

June 29-30- Arrived at Palo Alto and talked with various people at Stanford, including Spencer, Polya, Hirosky, and J. K. Vennard.

June 30-July 1- Returned to New York by plane.

Thursday, June 12-Sunday, June 15. Traveling by train. Wrote a paper and read Chapter IV of the manuscript on shock waves.

Monday, June 16.

Spent most of the morning getting in touch with the people I want to see, making travel arrangements, etc.

Just before lunch went to the L. A. Municipal Airport to North American Aircraft Inc. to visit the Aerophysics Laboratory. Met W. Bollay, but talked to him only very briefly as he had to leave. Dr. Randels took charge. I discussed their report on the water wave analogy for supersonic flow of compressible gases and asked questions about some points which puzzled me, but did not get very satisfactory answers. The people who use the water wave analogy seemingly do not care to investigate the fundamental basis for that theory---with some justification since they are not primarily interested in water waves but in gas dynamics. I then saw the actual experiments consisting in dragging an object through shallow water and observing the wave patterns. The angle for oblique shocks does not come out right unless the model is long enough, i.e. unless the "wave lengths" are sufficiently long compared with the depth. Had lunch with

Dr. W. C. Randels - in charge of investigating the water wave analogy.

Dr. A. Vanszonyi - pupil of Emmons

Dr. J. J. Gilvarry - physicist from Princeton

J. R. Bruman - makes the experiments on water waves.

At lunch we spent most of the time discussing Friedrichs's recent result that shocks cannot begin in the interior of a region in steady 2-dimensional supersonic flow. Vaszonyi was very sceptical, perhaps because I could not explain it too well.

Tuesday, June 17.

Up early and left L. A. for Del Mar, where I was met at the station and taken to the Scripps Institute. I met Dr. Walter Munk for the first time. W. M. is in charge of the research on water waves at the Scripps Institute. We discussed our work very animatedly for an hour or so until lunch, and decided that I should outline what our

group has been doing in the last year or so to a group of the people here in the afternoon. This was done. I talked for nearly two hours, with many interruptions for discussion, in the course of which we managed to clear up a number of points. The thing of principal interest to Munk and his people is the theory of breaking of waves, and they are very much interested in our approach, which differs from Munk's. Munk operates with the notion of the solitary wave, which I feel is wrong for reasons to be set down in a moment, while Munk correctly says that our method yields results not always in good accord with the observation on ocean beaches. A comparison of the two methods follows: Both methods work with nonlinear shallow water theories (these approximations to the exact hydrodynamic theory which are accurate for long waves in shallow water), However, I assume that the motion is not a steady motion and I am able to compute changes in shape of a wave moving into still water. The theory is exactly analogous to gas dynamics theory, and the formation of a breaker corresponds exactly to the formation of a shock in a tube. Munk reasons on the basis of a special steady motion furnished by the theory of the so-called solitary wave---a wave form which can propagate unchanged in form, at least in water of constant depth. The trouble is that this wave has a form which is symmetrical about the crest, and that seems to me to be rather wide of the mark for breakers in many cases---to my eye they seem to get very steep in front. (Our theory furnishes this behavior). On the other hand, Munk points out that the observations indicate that waves break as soon as the ratio of amplitude to depth exceeds a certain value---about 1.3---and that the solitary wave approach makes it possible to understand this at least in part, while my method indicates that the breaking phenomenon is practically independent of the amplitude-depth ratio. The fact is, however, that the two approaches are not so contradictory, for the following reasons. Both theories are approximations to the exact theory which arise essentially through developments with respect to the depth of the water. The theory used by Munk carries the development to terms of higher order than is the case for the theory I use, but assumes a steady motion and also one of very special character. Thus the theory I use

is less accurate, particularly in the immediate vicinity of the breaking point, but it has the virtue that one can obtain the unsteady motion which results under any prescribed initial conditions. For breaking of the type called a bore, our approach is without question superior to Munk's, but for waves on a beach it may not be accurate enough in some instances. What is needed is an investigation of the unsteady solutions which can arise when the terms of next higher order (above those in our theory, that is) are taken into account. However, this will be no triviality, since the analogy with compressible gas dynamics and shock waves would no longer hold and the method of characteristics would no longer be applicable.

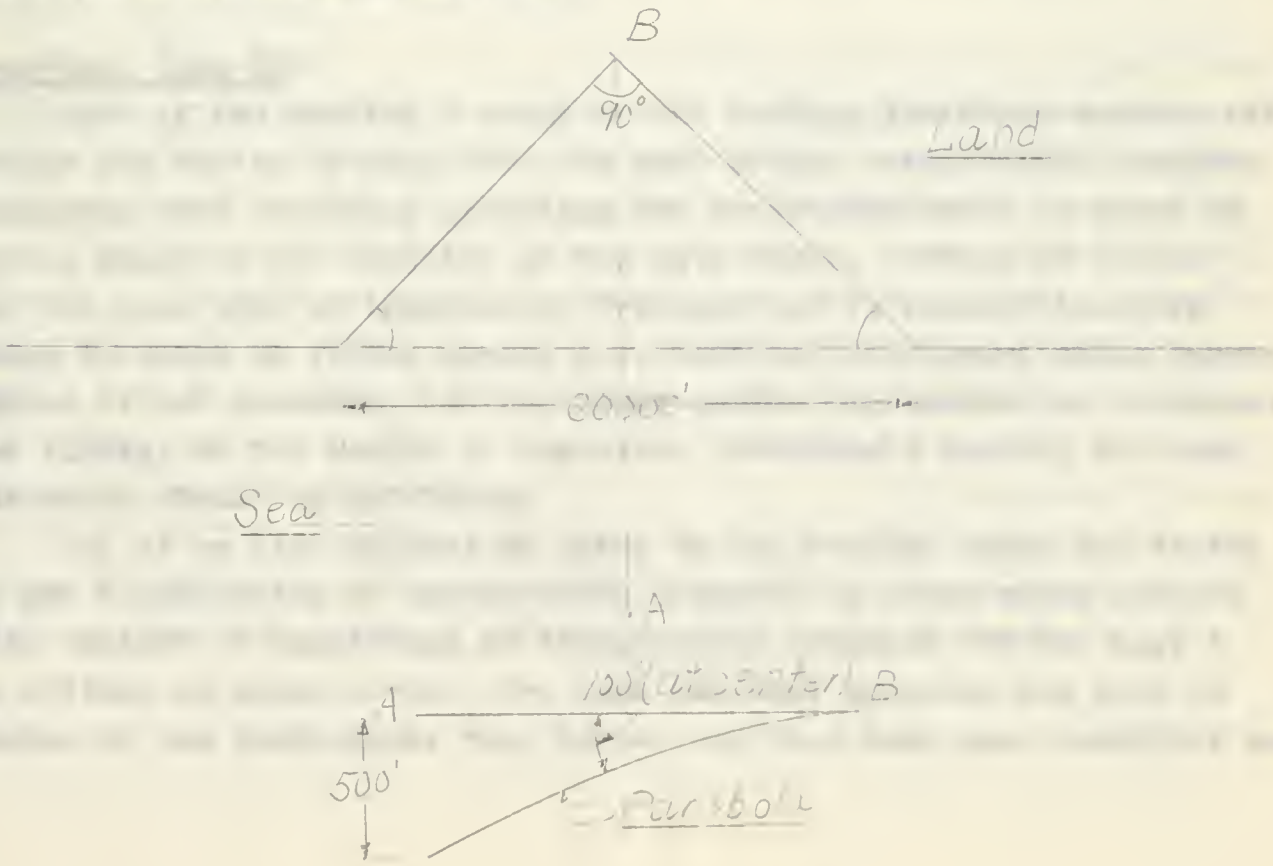
Wednesday, June 18

The first part of the morning I spent with Munk, who wanted to show me what he does with the solitary wave theory and to ask my advice on some purely mathematical points. We then drove to San Diego to the Marine Physical Laboratory to see Dr. Carl Eckart of the U. S. Navy Electronics Laboratory. Munk wanted me to tell Eckart about my theory of breaking, which interests him apparently. He seemed to feel that my ideas hit the nail on the head qualitatively at least. We then went on to talk about other things, in particular elasticity. Eckart has a new theory about residual strains and stresses which he wants to develop in order to deal with problems in a field which he calls anelasticity. A problem in this field arises from the following observed fact: If a wire is stretched suddenly by a weight, it of course stretches a certain amount quickly but if the weight remains on the wire it will apparently continue to stretch slowly, even though all stresses are well below the elastic limit.

Eckart spoke of Courant and said that he wanted to invite him to come out to visit and perhaps give some lectures. Eckart and his group want to expand, apparently, and are looking for people. They wanted Munk to leave the Scripps Institution to go there, but he seems not to want to do it. Just as in the East, there is a great demand for trained people at many places on the Pacific coast.

After lunch with Ewart we returned to Scripps and I talked with Dr. Sverdrup, the head of the Scripps Institution. Our talk ranged over a variety of topics, from water waves to general educational and research policies. I was surprised to learn that Sverdrup is returning to Norway as head of an Institute of some kind and will stay there permanently. He has been in this country eleven years. It is too bad to lose him.

After talking with Sverdrup I went to work once more with Munk and Arthur. They are at present much concerned with what seem to be called tsunamis---a Japanese word for waves in the ocean arising from earthquakes---and are studying the observations on the most recent one which originated in the Aleutians and caused considerable damage in parts of Hawaii. They hope eventually to learn enough to be able to make predictions, perhaps from the seismic records. They went on to calculate the wave amplitudes in the following special case and to devise methods which they could use numerically in similar cases. The case in question is that of a bay at Hilo (I think) which has roughly the following dimensions:



At the entrance to the bay a steady wave with a twenty minute period is to be assumed (and wave length corresponding to the depth there). The bottom is to be taken as cylindrical, with a parabola as generating curve and horizontal generators. There is no real difficulty, except in knowing the appropriate boundary conditions along the shore line. One might assume perfect reflection, perfect absorption, or partial reflection and absorption. It was decided to assume perfect reflection, so that the solution will be a standing wave.

At about 5 P. M. Arthur and I went swimming in the surf. The water was cold, but not as cold as it usually is at Jones Beach, Long Island, N. Y., and the waves were coming in quite high and with long straight crests. It was a pleasant change to swim in water waves rather than to talk about them. Although I have been out here four times, this was the first time I had been in the Pacific. After swimming, Arthur took me to his home for dinner, and I spent a very pleasant evening with him and his wife. Arthur would like to study with our group, perhaps a year from now, and I encouraged him to do it. He is the type of person we can help.

Thursday, June 10.

Most of the morning I spent at the Scripps Institute working with Arthur and trying to help him with some purely mathematical problems concerned with obtaining solutions for non-steady waves in water of finite depth in the vicinity of the wave front. Arthur of course had the same kind of troubles as Friedman had in calculating ship waves in water of finite depth, i.e. that the stationary phase approximation is not accurate, and the power series representation converges too slowly, at the region in question. Friedman's results on these questions should be published.

One of my main objects in going to the Pacific coast was to try to get a collection of photographic material on water waves under a wide variety of conditions as illustrative material for the book I am writing on water waves. Mr. Tuck was very generous and gave me copies of any photographs they had--- and they have very excellent ones.

later lunch I had just time enough for a swim in the surf and then I took a train back to Los Angeles, arriving there at about 6:30 P. M. I went to Pasadena to the Athenaeum at Cal. Tech., where one of our former co-workers at P. L. E., Dr. Charles Fairfax, arranged that I might stay.

Friday, June 20.

I talked to P. P. Popp for an hour or more about the work of our group on water waves in general, and the recent work on breaking of waves in particular. Popp was most interested in the part of this work that refers to the breaking of dams, and of flood waves in rivers and channels generally. It seems that there is even more practical interest in the subject of roll waves, which I had thought was more a mathematical curiosity, than anything else. But it seems that surges of this character may occur in some of the steep channels in the Los Angeles region in the rainy season and that the sides of the channels must be made high enough to contain the maximum surges. It may be that a part of the work we have in view for the future will throw more light on the subject of roll waves, which seems very mysterious at present.

Popp introduced me to Dr. K. O. Wagner, who works in experimental hydraulics. In particular, he seems to be in charge of the large and elaborate model of Cuba harbor (it is 70-100 ft. in diameter) which they are studying with a view to planning the breakwater designs in the inner harbor to protect the anchorages, dry docks, etc. from the effects of the wave action transmitted from the ocean into the harbor through the harbor entrance. The thing is done very elaborately. Popp says he considerable number of the photographs they have of the waves inside and outside the harbor, as obtained from the model. Some of these will be excellent for my book. I have strongly the impression that one could perhaps do a good bit of theoretical work on harbor design which might then lead to methods of calculation which would in turn make a lot of expensive model tests unnecessary.

In the afternoon I was introduced to M. S. Bloom and his wife by DePrima. Bloom was very much interested in Friedrichs's recent result proving that shocks cannot develop in the interior of a fluid. Bloom will probably visit us in New York in the course of the summer. I paid a visit to Courant. We talked mostly about nonlinear vibrations.

J. B. Keller (now at N. Y. U., but formerly at Cal. Tech.) is in Canada on his way to China where he is going for a visit of a few months. We talked for an hour in the afternoon, and agreed to meet later for dinner that evening. Keller, DePrima, Lagerstrom, and I had a very good dinner at a French restaurant in Pasadena and spent a pleasant evening together.

Saturday, June 22.

I took my first day off since I left New York and went to Long Beach for swimming with DePrima and a number of his friends. The swimming was good---though not as good as it is Solla because the surf was not high enough---and the weather was hot. In fact, I got my face pretty badly sun burned.

Sunday, June 23.

I flew from Los Angeles to San Francisco, arriving at noon. M. Lowe, E. Spencer (both of whom formerly worked with our group at N. Y. U.), and F. Shiffman (of our group, but visiting with his family at Stanford for the summer) met me at the airport. We went to a restaurant not far from Palo Alto for lunch, then went on to a place called the Big Basin where there is a beautiful grove of redwoods. Later that I arrived in Berkeley at about 10:30 P. M., I was exhausted and went to bed immediately.

Monday, June 24.

Practically the entire day was spent with the aeronautical people in hydraulics at the University of California at Berkeley. Most of the time I was with J. B. Johnson, Associate Professor of Mechanical Engineering, who works chiefly in the field of water waves in open

channels, in harbors, on beaches, etc. I had wanted to talk with Isaacson and Putnam, but they were busy. I described the work of our group in these subjects during the past year or so, and he was very much interested. In particular, the work on the breaking of waves and the associated problems concerning the breaking of a dam and the progress of a flood wave down a river or channel touches their experimental work very closely. In fact, Johnson suggested that they might try to make experimental quantitative studies of unsteady flows in open channels and compare them with our theoretical results. Naturally I encouraged this idea.

Johnson introduced us to Dean C. P. O'Brien, whom I had never happened to meet before, although I have known of his and his work's existence for some years. We had a lengthy conversation, mostly about unsteady flows in open channels. (Apparently this is an encouraging subject for the hydraulics people just at present, and may help on the theoretical side). An amusing incident occurred in connection with the problem of the breaking of a dam. O'Brien had calculated the shape of the wave and the motion which results on the basis of a somewhat involved argument and a number of specifying assumptions, and came out with the result that the wave is parabolic in shape and that the water height and velocity remain constant at the site of the dam although the motion as a whole is unsteady. To our [illegible] was therefore considerably surprised to learn that this is the exact result we obtain by integrating the partial differential equations of the nonlinear shallow water theory.

Dean O'Brien is taking leave of absence from the University and will be in New York for the next year, where he is acting as a consulting engineer. He will visit our project.

After leaving La Jolla I began to have some doubts about the formulation of the problem on water waves due to Friedrichs that was given to me by Hunt and Nathan (see above). In particular, the crucial point about the boundary condition along the shore line still bothers me. I discussed this with Johnson, and found (as with Hunt and Nathan) that I had a hard time to make him see what the problem is and what the mathematician's attitude is. (These people had not realized the necessity for a singularity at the shore line in the

linear theories if one wants to obtain a progressive wave rather than a standing wave). Johnson had in the end the opposite view to that of Arthur and Munk, i.e. he felt (as I am inclined to feel, too) that most of the incoming energy in a tunnel is destroyed at the shore line through the breaking of the wave. Johnson took me to talk with a young man named John D. Isaacs who is in charge of a project called Waves - Ocean - Beach - Survey and who had been in Hawaii to collect information about the recent tsunami there. He told me that the indications were that breaking occurred almost everywhere along the coast fronting the incoming wave and that most of the damage was caused by the first, or possibly the second, wave. In other words, the practically interesting case may well be that in which the unsteady initial motion is varied, rather than a periodic steady state; and, in addition, it may be closer to the facts to assume absorption of all incoming wave energy rather than perfect reflection. Some correspondence on this point with the people at La Jolla would seem to be in order.

I met in La Jolla and Cal. Tech. I saved for, and got, very excellent photographs of water waves from Johnson. I must get busy and get my book on water waves written.

I looked up W. R. Sears (until recently a member of our group) and talked to him about his work in the Engineering Division at Berkeley. He is working on a project concerned with flow of highly rarefied gases at supersonic speeds, and is very enthusiastic about the work. O'Brien had mentioned the possibility that Sears might help Johnson and his people on the theoretical side for some go flow in open channels and Sears promised me that he would do so.

Sears took me to visit C. C. Lyra, head of the Mathematics Department, whom I have known for some time. We talked mostly gossip and news about mathematicians and mutual acquaintances and also a little bit about some interesting problems in potential theory in three dimensions which have come up in some of my recent work on water waves.

One of the main objects of my visit to Berkeley was to talk about many problems with H. Lewy, who has and still does work with us on various water wave problems. However, he was married recently (after many years of bachelorhood) and was in no mood to talk mathematics, since he was hard at work planning a year's trip around the world with his bride.

<u>Tuesday, June 24 to Friday Evening June 27.</u>

Took a vacation in the Sierras. Went in a car with Dr. [illegible], who will join our group at N. Y. U. in September. We went first through Placerville to Lake Tahoe, then over Echo Summit over to Nevada and followed the eastern escarpment of the Sierras to Mono Lake. From there up the Tioga Pass (elevation over 9900 feet) into Yosemite Valley. Climbed Unicorn Peak (10,900 feet) and swam in Lake Elizabeth (9500 feet), which was really cold----it is fed by the remnants of snow on the mountains beside it. Then went down into Yosemite Valley proper. This is a sight that more than comes up to the descriptions. From there to Mariposa Grove, where there are hundreds of giant Sequoias ---in particular I drove the car through the one so often seen on photographs. These trees are truly magnificent and have to be seen to be believed. From Mariposa back to Berkeley on Friday afternoon---- a long hot drive. I had been in California three times before the present trip, but had never seen the high mountains before. They are wonderful, but not so superb as the Alps even though the Alps are on the whole not quite as high. However, the combination of Nevada desert on one hand and immense snow-capped ridges on the other is something quite wonderful which cannot of course be seen in the Alps.

<u>Saturday and Sunday, June 28, 29.</u>

I went to Palo Alto to visit people at Stanford. I stayed with M. Shiffman of our group, who, as previously mentioned, is teaching at Stanford this summer. I also talked with S. Bergman who has an ONR project devoted to certain researches on analytic functions. On Saturday evening I visited Professor and Mrs. Polya, whom I knew from the time I studied in Zurich. Since the beginning of the war,

Pelz has been at Stanford. He and Dean also have an NSF project concerned with calculation of the electrical capacities of various types of boilers. The pelz's were, of course, very much interested to learn all about our visit to Purdue last Saturday, particularly since I had been out talked with many of their old friends. It was a particularly pleasant evening.

Tuesday, June 20.

Most of the morning I spent with Dr. J. Minorsky, who conducts an NSF project on nonlinear vibrations, a subject in which Prandtl and I and others in our group as well have done research in former years. Minorsky wanted various papers and lecture notes of ours which are not available at Stanford. Minorsky has had some difficulties getting started at Stanford, but he feels that things will progress better now that he has two or three capable graduate students to work with him. He is planning to do quite a little experimental work on both electrical and mechanical vibrations. I mentioned a number of things which he would be interested to see investigated experimentally, for example the question of the relation between the magnitude of the damping and the relative area of production of subharmonic oscillations in a system with a nonlinear restoring force, and the extremely interesting question of higher order approximations (of Lagrangian type) to the relaxation oscillations. Minorsky had not heard of the work of Cesari in the latter connection--in fact the idea of considering asymptotic developments in the neighborhood of limit parameter values instead of perturbation series for small values of the parameters had not occurred to him. Minorsky told me of very special problems which interest him, some of which we might to learn about. For instance, there is a phenomenon in nonlinear resistance circuits, called often by the German name "Mitnahme" which refers to the fact that an oscillation can be controlled with very great accuracy (in the frequency, that is) under certain circumstances.

Minorsky also continues to be interested in systems leading to difference-differential equations (due to time lags in control elements of one kind or another). I learned also that part IV of Minorsky's series of articles published by the David Taylor Model Basin has

appeared. This one deals with relaxation oscillations, and we must get a copy of it.

Just before lunch I looked up J. K. Vennard, a former colleague from the College of Engineering of N. Y. U., who is now at Stanford Vennard works in hydraulics. It was too bad that I had so little time to talk with him since it seems that he also is taking an interest in the subject of waves in open channels and wants to try to develop experimental techniques for studying non-steady motions in this field of problems.

At 3:30 P. M. I left San Francisco by plane for Los Angeles, where I took a plane for New York which left at 6:30 P. M. and arrived in New York at 7:45 P. M. (both the local standard time). The plane was one of the new D. C.6's and was very steady.
I arrived home tired but contented in every way with the results of my trip. Things worked out better for me this time than on any other of the long trips I have taken.